LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Does Chemical Transformation Influence Weight?

CAREFUL experiments by Heydweiller, published in the last number of Drude's *Annalen* (vol. v. p. 394), lead their author to the conclusion that in certain cases chemical action is accompanied by a minute, but real, alteration of weight. The chemical actions here involved must be regarded as very mild ones, *e.g.* the mere dissolution of cupric sulphate in water, or the substitution of iron for copper in that salt.

The evidence for the reality of these changes, which amount to 0·2 or 0·3 mg., and are accordingly well within the powers of a good balance to demonstrate, will need careful scrutiny; but it may not be premature to consider what is involved in the acceptance of it. The first question which arises is—does the *mass* change as well as the *weight*? The affirmative answer, although perhaps not absolutely inconsistent with any well ascertained fact, will certainly be admitted with reluctance. The alternative—that mass and weight are not always in proportion —involves the conclusion, in contradiction to Newton, that the length of the seconds' pendulum at a given place depends upon the material of which the bob is composed. Newton's experiment was repeated by Bessel, who tried a number of metals, including gold, silver, lead, iron, zinc, as well as marble and quartz, and whose conclusion was that the length of the seconds' pendulum formed of these materials did not vary by one part in 60,000. At the present day it might be possible to improve even upon Bessel, or at any rate to include more diverse substances in the comparisons; but in any case the accuracy obtainable would fall much short of that realised in weighings.

As regards Heydweiller's experiments themselves, there is one suggestion which I may make as to a possible source of error. Is the chemical action sufficiently in abeyance at the time of the first weighing? If there is copper sulphate in one branch of an inverted U and water in the other, the equilibrium can hardly be complete. The water all the time tends to distil over into the salt, and any such distillation must be attended by thermal effects which would interfere with the accuracy of the weighing.

RAYLEIGH.

June 11.

The National Antarctic Expedition.

IN consequence of a cable received yesterday from London, telling me that the instructions for the conduct of the National Antarctic Expedition that had been passed by the Joint Committee of the Royal Society and the Royal Geographical Society have been greatly altered, I feel it my duty to resign the post of head of the civilian scientific staff of the expedition, which I had provisionally accepted. The organisation of the expedition now passed leaves the head of the civilian scientific staff nominally responsible for most of the scientific work of the expedition, but gives him no power to secure the performance of the scientific part of the programme.

The responsibility for my withdrawal at so late a date rests with those who have delayed until now the settlement of the programme and organisation of the expedition, which should both have been decided, as I understood they had been, before the ship and most of the equipment had been ordered.

I trust the protest of my withdrawal will secure to my successor more favourable conditions of work than the altered instructions would have given me.

University, Melbourne, May 4. J. W. GREGORY.

The Settlement of Solid Matter in Fresh and Salt Water.

SINCE the publication of the report of Mr. Slidell[1] on the deposits of the Mississippi delta, containing the remarkable statement that while the deposit contained in the river water of the Mississippi took from ten to fourteen days to settle, with solutions of salt, sea water or sulphuric acid the water became limpid in from fourteen to eighteen hours, it has generally been taken as an accepted fact that alluvial matter settles more rapidly in salt than in fresh water. Sir Archibald Geikie, in his "Text-book of Geology," endorses this theory; and in a recent article in the *American Engineering Magazine* on the transportation of solid matter by rivers, Mr. Starling, one of the Government river engineers, states that a small quantity of salt or other foreign material dissolved in water will diminish the suspending power and increase the rapidity of subsidence to a marked degree, sometimes even many hundred-fold.

On the face of it the result naturally expected would be, that as sea water is of greater specific gravity than fresh water, and more viscous, the grains of solid matter would sink more slowly in salt than in fresh water. The very great distance over which solid matter brought down by rivers remains in suspension after reaching the sea, extending from six miles from the mouth of the Rhone to thirty-five from the outlet of the Nile, up to 300 miles over which the sea water is stated to be discoloured by the effluent of the Amazon, appears to indicate that salt water is capable of retaining solid matter in suspension for a longer time than fresh water.

Experiments made by Mr. Vernon Harcourt with alluvial matter placed in suspension in sea water and fresh water, and in solutions containing different strengths of salt and other foreign material, although not of a conclusive character, show that there is little difference between the rate of deposit in sea or in fresh water. Of samples from different estuaries which were allowed to settle in sea water and pond water respectively, the particles of the former took about 9 per cent. more time to subside than the latter. The general conclusion he arrived at was that, though sea water promotes the deposit of "very light clayey matter contained in river silt under favourable conditions, there are no grounds for regarding it as exercising the very preponderating influence on the formation of deltas attributed to it by geologists."[1]

The writer some time ago investigated this subject in connection with researches he was then making as to alluvial deposits in estuaries, and has again more recently conducted a series of experiments the mean results of which are given in the following table:—

Table Showing Rate of Settlement of Solid Matter in Fresh and Salt Water.

No.	Number of grains to a lineal inch	Material	Time taken to settle		Water clear		
			Fresh	Salt	Fresh	Salt	
			m. s.	m. s.	h. m.	h. m.	
1	100	Sand	0 10	—	—	—	Water not discoloured
2	200	do.	0 25	—	—	—	do.
3	—	Whiting ...	12 0	—	0 30	—	
4	—	Plaster of Paris	5 0	—	0 10	—	
5	300	Warp, Trent	0 43	0 45	0 1	0 1	Water scarcely discoloured
6	1400	{Fine Warp, Dutch River}	12 0	15 0	3 30	22 0	
7	500	Silt, Salt Marsh	2 0	2 0	0 6	0 9	
8	1000	Warp, do. ...	8 0	9 0	1 0	0 33	
9	2000	{Alluvium, Boston Dock ...}	33 0	28 0	7 0	1 30	
10	600	do., R. Parrett	4 0	2 40	0 15	0 18	
11	1500	do., Tilbury Bsn.	18 0	18 0	10 0	9 0	
12	1600	Brick Clay ...	17 0	15 0	1 30	1 9	

It will be seen from this table that the rate of deposit depends on the minuteness of the particles in suspension, and varies nearly in proportion as the square of the diameter of these.

With sand and silt there was practically no difference in the rate of settlement in fresh as compared with salt water. When the particles of the solid matter were very fine, as in the case of what is generally known as mud or ooze, the rate of settlement was slightly more rapid in salt than in fresh water; but there was nothing to justify the conclusion arrived at by Mr. Slidell.

All the material was first screened through a sieve having ninety meshes to the lineal inch.

The proportion by weight of solid matter to water was that which was found to exist on the average of fourteen large rivers

---

[1] Report on the Mississippi River by Humphreys and Abbott, 1861.

[1] "Investigation on the Action of Sea Water in Accelerating the Deposit of River Silt" (*Min. Proc. Inst. C.E.*, vol. cxlii.).

© 1901 Nature Publishing Group

when in flood, or ·79 lb. to a cubic foot equal to 1/80th part in weight of the water in the tubes.

Both sea water and water saturated with ordinary salt were tried, the latter in the proportion of one pound of salt to a cubic foot. There was no appreciable difference between these.

The samples were placed in glass test-tubes 1 foot long and ½ inch in diameter, filled with clean water up to the ten inch mark.

The material was well shaken and incorporated with the water, and the time given for settling is that taken by the particles to settle through 10 inches and become visible in a solid form at the bottom of the tube, and when no more particles could be discerned as settling when the tube was held up to the light.

The column "water clear" is that in which the water in the tube had become sufficiently transparent for black marks on a white ground to be discerned through it.

Practically all solid matter had settled in the time given in the first column. The quantity deposited between the interval of "settling" and "clear" was almost inappreciable, but still sufficient to keep the water discoloured. With the specimens containing the coarser material the water became bright again in the time given in the second column, but with the very fine material intervals varying from two to three hours up to as many days elapsed before the water became as bright as it was before the solid matter was added, partly depending on the fineness of the material, but due more to the staining quality of some of the ingredients contained in the sample. Thus the material taken from Tilbury Dock Basin turned the water a black colour which took some time to clear. The salt water took much longer to become bright again than the fresh.

Samples were selected as fairly representing the material brought down in suspension by rivers, or eroded from the sea cliffs, and deposited either in the form of salt marshes or transported to the bed of the sea.

Thus numbers 1 and 2 represent the sand found on the foreshores of the sea coast and covered at every tide; 3 and 4, material derived from chalk cliffs; 5 and 6, the material in suspension in the rivers Ouse and Trent, of which the Warp lands bordering on those rivers are composed, 5 being the material first deposited and near the river, and 6 that further away where the water remains quiescent for some time; 7 and 8 represent the material of which salt marshes are composed, 7 being the silt deposited on the sand, and on which, when it rises to about neap tide level, 6·68 above ordnance datum, samphire begins to grow, 8 the finer warp deposited from about the level of mean high water to that of ordinary spring tides, or 10·21 to 13·34 above ordnance datum, on which salt water grass grows; 9 is alluvial matter chiefly derived from the erosion of clay banks, brought up by the tides and deposited in Boston Dock, whence it was dredged, elevated from the barges and discharged with a current of water on to low land, the sample being taken at the part furthest away from the place of deposit; 10 was taken from the "batches" on the banks of the river Parrett at about half-tide level of spring tides, or 13·67 above ordnance datum, where the finest part of the alluvium in the river settles and which is collected for making bath bricks; 11 was taken from Tilbury Dock Basin on the Thames when the water was being stirred up by the eroding pumps; 12 is from clay used for brick making; 30 per cent. of the particles of this material were from 1/800th to 1/1000th inch in diameter and the remainder smaller than this, the average size being 1/1600 of an inch.

W. H. WHEELER.

The Subjective Lowering of Pitch.

IF the subjective effect described by Mr. E. Hurren Harding (*ante*, p. 103) is of general occurrence, it is contrary to what one might expect from the observation of singers.

It is well known that persons with a good ear may sing flat, being unconscious of the defect, though they would notice it immediately in other singers. From this it seems that the singer's voice sounds *higher* to himself than to others, and yet it is *louder* to him than to any one else. Sharp-singing, on the other hand, is regarded as more indicative of a defective ear.

I have no large tuning-forks at hand, but with ordinary forks and the sound-board of a piano I find that, on bringing the ear close to the source of sound, the sense of pitch is not altered, though the elements of noise are added to the sound; and these elements consist mostly of vibrations of lower pitch, presumably the proper notes of parts of the auditory apparatus.

In connection with this subject it may be noted that, owing to the structure of the cochlea, vibrations of small amplitude affect chiefly the lower part of its spiral; and that as the amplitude increases (independently of pitch?) the concussion reaches further up the spiral, where the fibres of the basilar membrane are longer than in the lower part, and therefore more responsive to slower vibrations. F. J. ALLEN.

Malvern, June 9.

WITH reference to Mr. Harding's letter (p. 103), it would be interesting to know whether the effect he has observed with the voice, with tuning-forks and with organ pipes can also be obtained from a siren. G. W. HEMMING.

YES; such effect can be obtained from a siren. If a siren be so rotated as to give a note approximating to middle C, the note appears flattened when the ear is placed close to the instrument, such flattening being estimated by different observers (at different times) as from a semitone to a whole tone.

Normal College, Bangor. E. HURREN HARDING.

THE NATIONAL ANTARCTIC EXPEDITION.

NO answer, so far as we are aware, has appeared to Prof. Poulton's letter to the Fellows of the Royal Society on Prof. Gregory's resignation of the leadership of the scientific staff of the Antarctic expedition (of which we published a copy on May 23). We are therefore forced to conclude that the representatives of this Society on the Joint Committee are content (to use our own words) to let judgment go by default, and admit Prof. Poulton's statements to be substantially correct. Since that date, according to a second letter which we published last week, rumours have been circulated that the real cause of Prof. Gregory's resignation was not that which had been publicly stated, but domestic considerations. The dates given to Prof. Poulton's statements and extracts from letters written by Prof. Gregory (which documents we have been allowed to examine) show these rumours to be baseless, and how they have arisen is no less a mystery than that alteration in the minutes of a resolution passed by the Joint Committee on February 14, 1900, mentioned in Prof. Poulton's former letter. Prof. Gregory's position has been consistent and definite throughout. He accepted the offer of the post on certain conditions, which he believed himself (not unreasonably, in our opinion) to have made clear. On returning to England last December he found the situation had been altered. Though not liking the changes he decided to accept them, and naturally supposed when he left England last February that the arrangement, concluded the day before he sailed, would be final. On receipt of a cable message that it had been further modified (by the acceptance, in substance, of Mr. Darwin's proposition), his first impulse, as he states, when the news arrived was to send a telegram announcing his resignation; but, after reflection, he thought it wiser to await the receipt of particulars by letter. Then came the refusal of the Council of the Royal Geographical Society to accept the instructions, thus modified, the appointment of an arbitration committee, as we may call it, and their decision, which virtually endorsed the action of that Society. When Prof. Gregory was informed by telegraph of the last step he at once cabled his resignation. We do not see how he could have done otherwise. There was now, to use his own words, "no guarantee to prevent the scientific work from being subordinated to naval adventure, an object admirable in itself, but not the one for which I understood this expedition to be organised."

Prof. Gregory, some experts have pleaded, is unreasonable in his expectations; the rules of the Service necessitate the complete autocracy of the naval officer in command. We content ourselves with the reply that if

© 1901 Nature Publishing Group